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EPIDEMIA, CONTAGION,

AND

INFECTION,

WITH

THEIR REMEDIES;

AN ESSAY:

TO WHICH IS ADDED AN

ENQUIRY INTO THE NATURE OF THE MODE

BY WHICH

CHOLERA

IS PROPAGATED.

BY

BENJAMIN PHILLIPS.

LONDON:
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GREEN, PATERNOSTER-ROW.

1832.

646.

TO

ANTHONY TODD THOMSON, M.D. F.L.S.

PROFESSOR OF MATERIA MEDICA AND THERAPEUTICS IN THE
UNIVERSITY OF LONDON,

IN WHOM ARE FOUND

GREAT PROFESSIONAL ACQUIREMENTS

AND

UNWEARIED EXERTION

FOR

THE ADVANCEMENT OF SCIENCE,

UNITED TO

A LIBERALITY OF FEELING

AND

KINDNESS OF DISPOSITION

WHICH ENDEAR HIM MOST TO THOSE

WHO KNOW HIM BEST,

THIS ESSAY

IS INSCRIBED, WITH THE RESPECT AND ESTEEM OF

THE AUTHOR.

4, Welbeck Street, Cavendish Square,
January 1832.

PREFACE.

So long a period has elapsed since Europe was visited by a violent and fatal epidemic, that the steady progress and fatal character of the Asiatic Cholera, which has at length reached our own shores, has filled the public mind with alarm and dismay.

Originating at the Delta of the Ganges, in 1817, this disease has ravaged nearly the whole of Asia and a great portion of Europe; proceeding sometimes from town to town and country to country, in regular succession, and appearing, at others, simultaneously in distant districts.

Nourished into existence in a humid atmosphere acted upon by a burning sun, it has preserved its identity alike in the pestilential marshes of the Continent of India, in the sandy deserts of Arabia, and in the frozen wilds of the Northern provinces of Asiatic Russia.

Having radiated as from a common centre round the point where it first appeared, it has successively desolated the Continent of India, proceeding to the Southward as far as the Mauritius; the Eastern provinces of Asia, extending as far as China; the Northern provinces of Russia, reaching almost to Archangel; and traversed Westward to our own shores.

The history of the human race presents but few instances of any disease so extensively propagated, and so fatal alike in all climates; and we cannot, therefore, wonder that its progress has been watched with apprehension, its arrival hailed with dismay.

Man always magnifies what he imperfectly understands, and exaggerates the dangers he does not accurately comprehend. The object of the following Essay is to furnish correct information on a subject so deeply interesting to every one; and the Author trusts the unquestionable importance of the topics on which he has treated, and the sincere desire he feels to contribute, however slightly, to enlighten the public mind, will be deemed a sufficient apology for the defects, both of matter and manner, which the work may exhibit.

Cholera must either originate in atmospheric influence, or be produced by local exhalations proceeding from animal or vegetable decomposition: in either case, it may afterwards be transmitted from person to person by contagion.

The Author has, therefore, devoted a large portion of the following pages to an investigation of the cause and of the effects on the animal frame of ordinary and extraordinary changes of the atmosphere; the origin, the progress, and the effects of local exhalations; with the probable remedies for such exhalations when producing infectious disorders; and the laws which govern the transmission of disease from person to person, producing in the one a disease identical in character with that which existed in the other.

In pursuing this investigation, he has collected all the facts which appeared to him to bear upon those questions; has noticed the several epidemical diseases that, at different periods in the history of the human race, have traversed large portions of the old world; has shewn the fatal character of local exhalations—producing, as they do, when identical in character, diseases of a different kind in different individuals—such diseases being occasionally, and occasionally only, transmissible from person to person by contagion; and has examined the origin, the progress, the continuance, and the effects of those contagious disorders that have been most narrowly observed and most carefully investigated

It may be the opinion of some of his readers, that he has devoted too much space to the narration, and occupied too much time in the investigation, of particular diseases bearing, as may be said, no relation to Cholera; and others may reproach him with having pronounced no exclusive opinion of the character and action of Cholera. His answer, in both instances, will be alike. His object has been the collection of facts, the observation of cases, and the investigation of truth. He has not sought to build up an hypothesis. He does not wish to pronounce an oracular opinion upon a question hitherto, perhaps, insufficiently investigated.

CHAPTER I.

EPIDEMIA.

By the term Epidemia is understood any disease, however propagated, which attacks simultaneously a number of persons.

Epidemic diseases may be produced through the agency of contagion, of infection, of certain atmospherical changes, or of certain ingesta.

I shall here limit myself to the description of those epidemic diseases which are dependent on atmospherical influence, or certain ingesta; postponing the questions of infection and contagion to a later portion of the present Essay.

The atmosphere may present, as exciting causes, heat or cold, humidity or dryness, and a greater or less quantity of electricity.

The ingesta may be either small in quantity or deleterious in quality.

With respect to heat and cold, and their probable effects in producing epidemics, our data are totally insufficient to enable us to come to any satisfactory conclusion.

With regard to humidity and dryness, we are equally uninformed: and upon the question of electricity we have no accurate data.

From the time of Hippocrates, the nature of epidemics has been a no less fertile subject for ingenious discussion than that of contagion; and no part of the extraordinary works of the Father of Medicine has been more read, or given birth to a greater number of commentaries, than those chapters in which he has treated of epidemics.

Numerous as have been the discussions, and varied as have been the opinions, as to the causes under the influence of which epidemics arise, I am strongly disposed to believe, that the greater number owe their origin to some modifications in the condition of the atmosphere.

Although, as I have already stated, the observation of minute atmospherical changes yet remains to be made, we cannot be insensible to the fact, that changes occur in the condition of the atmosphere, and that the just combination of its elements, which produces in us a necessary healthy excitement, may be so modified as to excite in us disease.

Such an opinion was held by the great Sydenham, who said, "that the exterior conditions of man create a number of diseases which vary or disappear with the conditions by which they have been produced."

In the transition from winter to spring, for instance, and the consequent variations of temperature, a great number of acute inflammatory affections of the respiratory system are produced: what are these but epidemics? what is their exciting cause but a sudden change in the character of the atmosphere? The approach of autumn, again, is frequently ushered in by epidemic diseases: these are most commonly fevers, either intermittent or typhoid, which we suppose to be dependent, not simply upon the change of temperature, but upon miasmatic exhalations, which are abundant at this period.

Those diseases which are epidemic at certain seasons, and which almost constantly appear in those seasons, are, I apprehend, dependent ordinarily on atmospherical changes, perhaps upon change of temperature alone.

We have seen, that, when measles has become epidemic, most other diseases occurring at the same period were complicated with a catarrhal affection similar to that existing in measles.

When epidemic diseases have acquired considerable intensity, other diseases which ordinarily exist at such places at the period become less frequent.

This circumstance has been particularly noticed in those countries where epidemic diseases occur frequently. Prosper Alpin says, that, in Egypt, those diseases which generally exist sporadically in that country cease upon the appearance of the plague.

During the epidemic which existed at Moscow, toward the conclusion of the last century, Orraüs states, that, while this epidemic reigned, there was no case of small pox seen; but that the latter reappeared as soon as the prevailing epidemic disappeared.

Shnurrer states, that, in the East, the appearance of small pox while the plague reigns is a subject of rejoicing with the natives; for they have experienced, that it is an infallible indication that the pestilential epidemic is about to terminate.

Inexplicable as it may be to us, we are taught by observation, that, at certain periods of the existence of the human race, as also at certain epochs of our individual lives, man suffers changes in his organization, and modifications of a more or less profound character, from which, to a certain extent, may result the periodical manifestations of different diseases which affect simultaneously a great number of individuals.

We see occasionally a disease attacking a great number of individuals almost at the same time. This epidemic may be only an extension of a disease which, in that country, has ordinarily presented a sporadic character: such as the cholera in India. It may, however, be a disease scarcely or not at all known in that country, or indeed in any country: such was the disease which existed in Paris as an epidemic in 1824-5, and which was termed acrodynia.

It must be evident, that neither our observation of atmospherical changes, nor our knowledge of electrical action, is such as to enable us to determine how far epidemics may occur simultaneously with hygrometrical or thermometrical variations; and that we are not possessed of data sufficiently accurate to determine how far certain kinds of food may predispose to the development of epidemics.

To be enabled to determine the part which is performed by atmospherical changes, in the production or propagation of disease, observations should be made in every town and every district, to determine, first, the mean temperature of that town or district, the variations to which it is subject, and also the extremes of temperature which may exist at that spot.

Arago, to determine the mean temperature at Paris, observed the temperature every morning between eight and nine o'clock. The temperature of this hour represents very nearly the mean temperature of the day. In elevated latitudes (say from the North Cape to Cairo), it has been found that the mean temperature of October is nearly that of the year.

It is also necessary that we should observe the influence of abrupt changes of temperature upon the human body. A series of observations, to determine this point, should be made in those countries where the difference between the temperature of day and night is considerable: thus, at Senegal, between morning and midday there is a variation in temperature of twenty to thirty degrees.

Again, between summer and winter, in latitudes between thirty and seventy-eight north, the mean difference of temperature between these seasons is twenty degrees; between the latitudes of thirty-eight and twenty north the difference diminishes, being ranged between ten and twenty degrees; and, between the latitudes of twenty and the equator, the difference never exceeds ten degrees.

There must be peculiarities dependent upon the circumstance, that man is modified so as to exist in Pekin, where the change is thirty-four, and at Cumana, where it does not exceed two degrees.

With respect to the influence of heat and cold, unaccompanied by great variations, we find man subjected with impunity to a heat of +46 centigrade, and a cold of —50.

It should be observed, too, that the minimum of heat in a day is at about an hour before sun-rise, and the maximum at about two o'clock in the afternoon.

Until the observations necessary to construct accurate atmospherical tables shall be completed, our knowledge of the effect of heat, of cold, and of variations of both, upon the living body, in the production of epidemics, must rest stationary.

It is doubtful whether much influence is exercised upon the nervous system by an elevated temperature; it has, however, been observed, that, although in a disease existing epidemically in a temperate country, the nervous system may not be affected, yet, if this epidemic extend to a hot country, the nervous system will be found to participate with the rest of the system in the effects of the disease. This circumstance frequently produces, in an epidemic existing in a warm country, a character and a gravity which it did not possess in a temperate country.

It has also been observed, that certain portions of the human frame are more or less affected, according to the temperature to which the person is exposed.

There is a disease affecting mucous membranes which presents a great singularity: I mean ophthalmia, which occasionally becomes epidemic: and the most intense epidemics of this disease have heen developed in two extremes of temperature; amongst the sandy deserts of Africa, and on the icy seas. In this disease, I apprehend, the cause is not so much the existence of heat or cold as the transition from the one to the other. At Paris, from 1820 to 1827, ophthalmia was observed to be epidemic, or at least to acquire its maximum of frequency, first in those three months in which the temperature is most variable, March, April, and May; secondly, in the two months in which the temperature is the most elevated, but in which the variation between day and night is very considerable, July and August.

Other observations, made in Belgium, shew that there also ophthalmia is most frequent in spring and during the greatest heat in summer.

With respect to those diseases of the digestive system which, under certain circumstances, become epidemic, it has been observed, that, as the temperature changes, the symptomatic form of the disease changes also, but its seat remains the same.

The kind of intestinal irritation which constitutes dysentery coincides most frequently, in its epidemic state, with an elevated temperature, either permanent or alternating with a low temperature.

Of fifty principal dysenteric epidemics observed in Europe, thirty-six reigned in summer, twelve in autumn, one in winter, and one in spring.

Of 13,900 persons attacked with dysentery in Bengal, from 1820 to 1825, Annesley found that there were 2,400 during the cold season, 4,500 during a hot and dry season, and 7,000 during a hot and humid season.

It has also been observed, that the general symptoms which are united to the different shades of intestinal irritation become more varied and more serious as the temperature becomes more elevated; and it is in this latter state that the nervous system becomes sympathetically affected, and the latter disorder may become so predominant as to mask the intestinal affection, and to lead persons to mistake the original seat of disease.

In the absence of observation to determine the influence of electricity in the production of epidemics, I have few remarks to make on that branch of the subject.

That electricity is an extremely powerful physical agent no one will deny: we know the influence which atmospherical electricity exercises upon living beings, producing uneasiness, depression, and anxiety, as many persons experience, in an extreme degree, during the existence of a thunder-storm.

It is not rare to see intermittent fever developed and assume an epidemic character in sailors who have encountered a thunder-storm in equatorial seas.

Atmospherical electricity is at its maximum in situations where vegetation is rank and there is much consequent evaporation, or where a quantity of water covers plants, or where the soil is strewed with the remains of animals or vegetables in a state of decomposition.

In these cases some difficulty would necessarily arise in determining whether electricity or the infection developed by this decomposition had the greater influence in the production of diseases arising in its vicinity.

Food may exercise some influence in the production of epidemic diseases: it may cause them, by its action upon the digestive tube itself, in irritating the mucous membrane; which appears to be the effect occasioned by unripe fruit in the production of dysentery; or the food may not stimulate sufficiently the digestive tube. From this cause diarrhœas frequently arise.

There are other cases in which the epidemics produced by aliments do not depend upon the direct influence which they exercise upon the digestive canal, but upon the action which their principles, when mixed with the blood, exercise upon the different organs.

Such is, I apprehend, the effect produced by the secale cornutum, or spurred rye.

In many parts of France, rye is the principal article of food of the poorer classes of the agricultural population. During very wet seasons, it has been found that this spur or ergot is developed. Its nature is not well known. Decandolle supposed it to be a parasitical fungus; Leveillé, Geoffroy, and De Jussieu, an unfecundated ovary: others have attributed it to the puncture of certain insects, which make it for the purpose of depositing their eggs.

Whichever of these opinions (if either) be the correct one, it appears that a wet season is necessary to its production.

Bread formed from rye is the usual article of food of the poorer classes, and as, during some centuries, no idea existed of the deleterious quality of the ergot, it was equally ground with the rye and used as food.

The use of such food produces a tendency to gangrene of the extremities; and, in wet seasons, the greater part of the rye being so affected, a disease of an epidemic character is generated, which has produced great ravages at many periods, and which, being uncommon, was, until 1630, supposed to be plague.

The epidemics of this kind in France, which have been most aggravated, have occurred in the years 945, 994, 996, 1090, 1096, 1099, 1109, 1128, 1130, 1140, 1234, 1373, 1630, 1648, 1674, 1675, 1702, 1716 to 17, 1764, 1781, 1818.

By an insufficient alimentation, the losses of the system are not repaired; and by this means dropsies are produced, and may become epidemic.

Such are the narrow limits which confine the knowledge we possess of the influence of the atmosphere, and of alimentation, in the production of epidemics.

For the purpose of endeavouring to throw some light on the propagation of cholera, I shall mention some circumstances connected with the principal epidemics which have appeared in Europe.

The plague of Athens, so effectively and affectingly described by Thucydides*, did not reach Greece until it had passed successively through Lybia, Egypt, Persia, and the Grecian Archipelago.

In the sixth century the greater part of Europe was desolated by a plague, which originated in Egypt and continued its ravages in Europe for a period of fifty-two years. Although we may refer its propagation to contagion, yet occasionally much difficulty is found in establishing the principle.

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* De Bello Pelopon. lib. ii.

From 1348 to 1386, Europe was ravaged by another pestilential disease, described as the "black plague," which, proceeding from China, passed through Russia, Poland, Germany, France, Italy, Sicily, the coast of Africa, the islands of the Mediterranean, and Spain.

The mode in which this disease was propagated, was undoubtedly by contagion. The gradual but steady progress of the disease, and the routes along which it passed, confirm, in the most complete manner, this opinion.

The striking number of analogies in the mode of propagation of this disease and cholera, would go far to establish an analogy in principle, and therefore the existence of contagion. That point being admitted, the observations which have been made for the purpose of neutralizing the influence of contagion will be rendered available in cholera.

In 1600, Europe became a prey to cholera; the origin of which is undetermined. The prevalent idea was that it was contagious; but the descriptions transmitted to us are any thing but exact and definite.

This disease occasioned death before the fourth day; and, until the present period, that was the only epidemic of cholera which has passed through the whole of the continent of Europe.

Other partial epidemics of cholera have existed. We find one described by Sydenham as existing in London from 1669 to 1672; and, although its symptoms were extremely similar to that now existing, it does not appear to have ravaged any other portion of our island. In 1729 and 30, appeared a pulmonary epidemic, which passed through Prussia, Poland, Hungary, Germany, Sweden, Denmark, France, England, Italy, and Spain. In 1775, the whole of Europe was visited simultaneously by a similar epidemic, described in Germany by Stoll, in France by Vandermonde, in England by Heberden and Pringle. Both were supposed to have originated in peculiar states of the atmosphere, but were afterwards supposed by many to have become contagious.

It is quite evident that when a disease appears, almost simultaneously, over such a vast extent of country, it must be produced by some atmospherical modification. At present we have cholera existing under almost every change of temperature, from the extreme of heat in the torrid zone to the extreme of cold in the temperate, in greatly elevated situations, and in those of an opposite description.

This is a peculiarity not often met with. We find other diseases frequently assume an epidemic form, but limited to certain latitudes. Thus with yellow fever, which has not been known to exist as an epidemic in southern latitudes.

Andral shews that of 196 epidemics of yellow fever, which have occurred in the last three centuries, none have occurred between the equator and the fourth degree of north latitude, 106 between the fourth and the thirteenth degrees of north latitude, 76 between the thirteenth and fortieth, 13 between the fortieth and fiftieth, 1 between the fiftieth and sixtieth, and none farther north.

When a disease commences simultaneously in a great number of situations, removed the one from the other, many of them having nothing in common but the circumambient atmosphere, we can have little doubt that its development is dependent neither on contagion nor infection, nor upon a combination of both, but upon some peculiarity of the atmosphere.

If, however, a disease proceeds regularly over an immense extent of country, we are not justified in presuming that foci of infection shall be conveniently placed in every situation for the purpose of developing the disease. If, too, it be slow in its progress, passing from town to town, from district to district, frequently too opposed by the current of air, we cannot conceive the possibility of such a gradual and regular change in the atmosphere.

CHAPTER II.

INFECTION.

I distinguish Infection as that principle which, emanating from animal or vegetable substances in a state of putrid decomposition, is capable of producing disease in a living body submitted to its influence.

The principle of infection may occasion different effects in those persons who are placed within its influence, and produce in them different diseases, and this to whatever cause the principle itself may owe its origin.

It has been very frequently stated, that all infectious emanations are identical; but chemistry has taught us the incorrectness of this opinion.

If the source of the emanation be in one case animal, and in the other vegetable, differences must necessarily exist, in the origin, the progress, and the effects of decomposition. Can it be supposed, that those emanations which present odours so different, or that those principles which cause diseases so opposite, which develope, in Egypt, the plague, in the Antilles, yellow fever, in India, cholera, in Italy, typhoid fevers, can be identical? Should we not rather admit, that these emanations, which we can neither see nor know, present, in their nature, differences not less numerous than we find manifested in their effects?

But, however mysterious may be the nature, and however diversified may be the effects, of infectious emanations, we cannot fail to observe a coincidence, almost uniform, between the existence of those manifestations and the manifestation of humidity in the atmosphere.

Under certain circumstances, a humid atmosphere and infectious emanations appear and disappear simultaneously, both originating in the same causes, or the one rousing the other into an activity more or less fatal to human life.

It is in the middle of the day, and in consequence of the action of the sun, that the air possesses least humidity: it is then that putrid emanations are least offensive and dangerous.

It is in the morning, in the evening, and at night, when, in consequence of the depression of the temperature of the atmosphere, the water contained in the air is condensed and produces fog, that putrid emanations are most noxious: it is then, too, that most frequently infectious diseases are manifested.

We conclude, from these facts, that it is not alone to the air, but to the water which it contains, that these emanations are principally united, and by which they are principally diffused.

The dispersion of emanations presents little uniformity; but, as a general rule, it may be stated, that the dangers attendant upon a focus of infection are dependent upon our proximity to that focus; in other words, if left to themselves, we may expect that infectious emanations will radiate around the spot to which they owe their existence. The wind, the conformation of the district, and perhaps other circumstances, at present unknown to us, may cause a diffusion of these emanations to a greater or less distance from their source, and distribute them more in one direction than another.

If the infectious principle exist in a circumscribed situation, its effects will seldom be manifested beyond these circumscribed limits; if in a valley surrounded by mountains, the emanations will probably be confined within those natural boundaries. Without being so circumscribed, they may be extended occasionally to a considerable distance. It is said that the odour of turf burnt in Westphalia can be distinguished at Brussels, Leige, and Paris!

A mountain or a forest may interrupt their course. Upon such a circumstance, according to Lancisi, was dependent the character for salubrity that Rome so long preserved; and it was not until a large forest, that intercepted the wind which had blown over the Pontine Marshes, was cut down, that Rome became insalubrious.

There is an idea prevalent in the present day, that insalubrious emanations are more disposed to ascend than to be diffused over a large extent of country.

It has been estimated that, in calm weather, putrid emanations, whether proceeding from animal or vegetable decomposition, do not ascend to a great height above the spot upon which they are generated, nor extend far from their source.

Of the accuracy of such calculations doubt must be entertained. They are based upon the experiments of M. Rigaud, made at the Pontine Marshes. According to the statement he has made, Sezzia, the elevation of which above these marshes does not exceed 1,000 feet, is altogether exempt from their exhalations.

Humboldt states that Encero, which is situated near Vera Cruz, and at an elevation of 3,000 feet, marks the highest limits of yellow fever in that district.

Immunity from the effects of such emanations is not, however, always enjoyed even in places having an altitude much superior to Sezzia or Encero.

We may reasonably conclude, that they are not readily dispersed, and that their physical character preserves us from the dangers that would attend their extensive diffusion.

The exhalations from vegetable putrefaction are charged with carbonic acid gas; those proceeding from animal putrefactions with nitrogen gas, hydro-sulphuret of ammonia, &c.

Both being heavier than the atmosphere, must have a natural tendency to preserve their original locality, and to resist the action of those agents which occasion the constant circulation of the atmosphere.

Their noxious quality may be, to a certain extent, estimated by their concentration. When extensively diffused through the atmosphere, their capacity to infect, if not wholly destroyed, is greatly lessened.

Whatever may be the height to which infectious exhalations ascend, the following narrative shews that they are not generally diffused over a large extent of country.

In 1800, when it was reported that yellow fever had manifested itself in Cadiz, 14,000 persons fled from that town and took refuge in the surrounding district, where they were exempt from the epidemic; while of 57,499 persons who remained in the town, 48,520 were attacked by the disease, and 7,387 died.

On the 1st of September 1803, when the yellow fever made its appearance at Malaga, the population of that town consisted of 51,745 persons; of whom 3,730 emigrated and escaped the epidemic; while of the 48,015 who remained in the town, 16,517 suffered from the disease, and 6,884 lost their lives.

On the 1st of July 1804, when the same disease again appeared in that town, the population consisted of 36,008 souls, exclusive of the garrison, the prisoners, and some other persons, altogether amounting to 3,500 or 4,000 individuals. Of the 36,008 persons, 4,548 took to flight and were exempt from the yellow fever; whilst of the 31,460 who remained in Malaga, 18,787 were attacked by the disease, and 11,486 died.

At the same period, at Alicant, there were, before the epidemic made its appearance, 13,957 inhabitants; of these 2,110 sought refuge in the adjacent country and escaped the disease; but of the 11,847 who did not quit the town, 6,947 experienced the epidemic, and 2,472 lost their lives.

During the epidemic which attacked Tortosa in 1821, nearly 5,000 persons sought refuge in the suburbs of Roqueta and Jesus, which are situated on the opposite side of the Ebro. These persons, although suffering many privations, were protected from the disease, which destroyed 2,356 of the inhabitants, out of a population of only 5,000 persons then within its walls.

At the commencement of the epidemic of 1821, which attacked the Barcelonese, 80,000 persons quitted the town; of whom some were already suffering from the epidemic. With these exceptions, the refugees were all exempt from the reigning disease. The refugees from Barcelona experienced every kind of vexation: the surrounding inhabitants and the adjacent mountaineers took every possible precaution to avoid them.

In spite of all the hardships to which they were subjected (not the least of which was an insufficiency of food), these unfortunate fugitives were exempt from the epidemic; whilst of the 70,000 persons who remained in the town during the calamity, 10,000 died. If, instead of flying, all had remained within the town, we may fairly conclude the number of deaths would have been doubled.

It is quite certain that some of these towns were rendered insalubrious in consequence of local exhalations: for instance, the celebrated epidemic of Marseilles was not propagated by contagion alone: many thousand bodies lying in the streets, as well of human beings as of domestic animals, and the accumulation of every kind of filth, contributed to render the disease much more fatal than it would otherwise have been. Indeed, it appears to be well established, that, in consequence of the extreme want of cleanliness, the disease had existed in the town six weeks before the arrival of the vessel commanded by Chataud, by which, it was maintained, the disease was imported. These circumstances, however, were not noticed in all these towns; and, whether the disease which ravaged those places were contagious and imported from other places, or were generated by local exhalations, removal and isolation would, in either case, have been effectual in arresting the progress and terminating the ravages of the disease.

The conflicting opinions which exist with regard to the nature of contagion and infection were, in some of these cases, productive of serious inconvenience.

If we look to the accounts of the progress of that frightful disease which, in 1821, desolated Barcelona, we shall find this inconvenience strikingly apparent.

For a long time, this disease was supposed to have originated in contagion; but the opinion is now general, and can scarcely be doubted by any one who has carefully considered the elaborate researches and observations of Chervin, that it was dependent upon local infection, producing a disease which might, under certain circumstances, become, and I believe actually became, contagious in that place.

That opinion receives ample confirmation from the circumstances attendant upon the yellow fever at Barcelona, at Cadiz, at Malaga, at Alicant, and at Tortosa, and from the cholera which broke out, in 1783, at Hurdwar, which was, no doubt, dependent upon infection existing at this place, where the Ganges takes its source, and which the Hindoos regards as sacred ground. To this spot they resort at a certain period of the year, in large multitudes, for the purpose of performing ceremonies enjoined by their religion; and, at certain epochs, which occur once in twelve years, the number of devotees is greatly increased. The year 1783 was one of these epochs, and the number of pilgrims was then immense. They were crowded upon the borders of the river, where they pass the night, ill clad, ill nourished; were exposed, without shelter, to a humid atmosphere; and indulged in all the excesses of debauchery. At this time, and under these circumstances, cholera attacked them; and, in less than eight days, the disease is said to have numbered 20,000 victims, although so little extended beyond the spot upon which it originated, that it did not reach the village of Juwallapore, which was only seven miles distant from this hideous scene. The disease terminated with the Hindoo ceremonies; for, although those who survived its ravages were dispersed over the continent of India, the disease was not propagated beyond the locality in which it originated.

If, again, we contemplate the epidemic which reigned at and in the neighbourhood of Mecca, and which commenced on the 15th of May last (an epoch when the Mahometans resort to that place from the surrounding country, for the purpose of offering sacrifice), we find that its invasion was almost instantaneous: individuals in a state of health fell down upon the ground, vomited, and died upon the spot.

The first thought that occurred to these people was, that the disease was the plague; an idea which was, however, carefully repulsed by the ulemas and scheicks, as well as by the Mussulman physicians, upon the faith of an article in the Koran, which says that the plague has been for ever exiled from their sacred places by the Prophet, and that it can never again enter them.

On enquiring into the origin of this disease, we find a general disposition to attribute it to a scarcity of water.

In the month of April, the heavy rains set in and produced torrents which destroyed the conduits by which water is conveyed to Mecca; and the city, encumbered too by an immense accession to its population, was thus extremely ill supplied with that necessary.

The physicians who resided at Mecca, however, maintained that this was not the only cause of the disease.

Previous to and during the time that these pilgrims were assembled, the state of the atmosphere presented, it is said, sufficient cause for the development of the disease. The heat which constantly maintained the thermometer at 102 degrees of Fahrenheit, the heavy rains which had occasioned an unusual humidity of the atmosphere, and the constancy of the South and South-East winds, acting as those causes did upon persons crowded in small spaces, badly clothed and badly fed, would satisfactorily account for the production of this dreadful scourge.

The extraordinary fatigues to which these numerous devotees submit in the performance of what they consider the sacred duty of visiting the tomb of the Prophet, render them peculiarly susceptible to the influence of disease. The place which they visit, for the purpose of performing their ceremonies, is an arid mountain, situated at a short distance from Mecca, without a single object to intercept the rays of a vertical and burning sun.

Europeans would scarcely credit a description of the mode in which their superstitious ceremonies are performed. During the three days specially consecrated to their religious duties, they assemble at the mountain called Arafata; and the immense concourse of pilgrims crowded together do not, during this time, stir from the place.

In the course of the third day, these unfortunate beings were almost inundated by the torrents of rain, which fell without intermission; but still the obligations of their religion would not permit them to seek shelter, as that day was devoted to prayer for our first parents after their expulsion from Paradise.

The number of deaths on the first two days of these ceremonies was considerable; but was increased with frightful violence during the third; and, whilst the rain was heaviest, death followed the attack with alarming rapidity, and the bodies of the dead remained without sepulture.

To the ceremonies of this the third day succeeded the fête of Mina, which was accompanied by scenes still more disastrous; and the dreadful mortality which followed was proportioned to the causes which gave it birth.

At the fête of Mina every Mussulman is accustomed to sacrifice a sheep; and it is said that thirty thousand of these animals were destroyed in one day: the blood, the entrails, and the remains of their carcases, were left to putrify; and the exhalations from the dead bodies lying at Arafata were wafted by the wind to Mina. All these sources of disease and death, thus increased to an extreme intensity, desolated, and still desolate, that unfortunate country.

The disease passed through all its stages in the space of two hours, sometimes of one; and at the termination of that period death almost invariably seized upon its victim.

We must bear in mind that numbers of these pilgrims arrived from infected countries, and may have imported the disease, which was afterwards dreadfully aggravated by the circumstances that have been detailed.

To these cases I will add one more. A vessel, the Fils de France, sailed from Nantes in 1826, with her crew in good health, and no disease was manifested on board until after they had cast anchor in the Ganges. In ten days after casting anchor, it was found necessary to repair the vessel: she was placed in a basin, on the right bank of the river; the basin was then drained of its contents, and the vessel lay upon the mud. The heat of the sun, acting upon this mud or slime, produced a putrid fermentation, the exhalation from which, in eighteen hours, affected the men placed under its influence: cholera was produced, which attacked indiscriminately the most vigorous as well the most feeble of the crew.

On the 16th of May 1817, a detachment of 90 men, of the first battalion of the 26th Native Infantry, proceeded from their former station to join the camp at Sangar. In the middle of an ordinary march, the detachment halted midway. The situation they chose was the border of a lake, about three miles in circumference, where they were shaded by trees; it was situated in the centre of a large open plain, surrounded by small hills.

All these men were in perfect health at the approach of night: during the night they were attacked by cholera. The first patient was attacked at midnight: he died in half an hour; and before sun-rise twenty-four men were suffering from cholera.

Although the detachment was removed to Sangar, yet, by the end of the week, all who remained of the detachment were in the hospital suffering from the disease.

These men were mixed indiscriminately with the troops at Sangar; but the disease was not communicated to a single individual.

No situation could be more favorable to the production of infection than the resting-place chosen by the soldiers.

In whatever manner these several diseases may have been occasioned, to whatever causes their origin may be attributable, whether they were conveyed from a distance, or were generated in the specific locality which they so fearfully ravaged, whether they may have been in their nature contagious or non-contagious, one thing I conceive I am justified in assuming—that in these particular localities there existed either infectious emanations, or some other peculiarity of the atmosphere, extremely favourable to the development of disease. And am I not also justified in supposing, that, however and wherever a disease may have originated, the greater or less violence which marks its progress will be dependent on the more or less favorable condition which the place, the atmosphere, the situation or the habits of the people, may present.

To illustrate this point, I take two towns, Hamburgh and Altona, in each of which cholera exists; but the comparative number of deaths in these towns bears no proportion the one to the other. Again: at Saratoff it was extremely fatal; in Moscow, much less so; dependent solely, as I apprehend, on the difference of salubrity in the two places.

Can we account for the insalubrity of Egypt in the present day, as compared with that salubrity for which it was so renowned in ancient times, in another manner than the neglect of inhumation?

In ancient times, the inhuming all domestic animals, as well as human beings, in sepulchres far removed from towns, was, no doubt, a powerful means of preserving the salubrity of the country, which, without such precautions, would have been, as it now is, a fruitful source of putrid pestilential emanations. In the present day, the places of sepulture are in the interior of towns, often in the interior of houses, whilst the bodies are scarcely covered with soil. Naked bodies, in a semi-putrid state, are seen in almost every street, and diffuse through the air putrid deleterious emanations, by which plague in the present day is endemic in that ill-fated country.

These circumstances, combined with the extreme filthiness of the inhabitants, and the confined character of their habitations, have tended to produce, in a country in appearance so beautiful, and the salubrity whereof was so highly vaunted by the ancients, that fearful and horrible disease.

We assume that these putrid emanations have caused this disease, because the plague is a modern disease; it was never seen in ancient Egypt, and did not appear there until the sixth century; a period subsequent to the introduction of Christianity into that country, by which all the ancient habits of the people, especially those concerning sepulture, were changed.

The disease mentioned by the ancients under the name plague, was undoubtedly typhus, caused by the emanations produced by the action of the sun upon the depositions left by the Nile after its overflow, containing, as they do, so many animal and vegetable remains.

The emanations from such a source may have produced typhus, and, aided by the more noxious exhalations proceeding from the decomposition of the bodies of men, as well as of animals, would become capable of occasioning a much more formidable disease.

It is from Egypt that every visitation of that scourge, with which Europe has been afflicted, has proceeded. Egypt then is the cradle of the plague; and it is in Egypt that we should enquire how it originated, how it is nourished, how propagated, and by what means we may hope to correct, if not destroy, one of the most frightful scourges with which Providence has ever yet afflicted humanity.

If the inhabitants of that country were made sensible of the necessity of cleanliness, and of resorting to their ancient mode of sepulture, much might unquestionably be done in mitigating the ravages of this, one of the most extensively fatal of diseases. Without these precautions, their soil, alternately covered by water and parched by a burning sun, will always be a focus where the emanations proceeding from putrid decomposition will be collected, and where the plague will continue its fatal ravages.

I shall take the plague as an illustration of a contagious disease; because we have authentic data to guide us in determining the laws by which the propagation of this disease is governed: and it is now generally admitted, that, to whatever causes its origin may be owing, it presents, under certain circumstances, a decidedly marked contagious character.

CHAPTER III.

CONTAGION.

We admit Contagion in all diseases in which the body of the person affected produces a principle capable of communicating the same disease to a healthy individual, without reference to the primitive origin of that principle, the circumstances which render impregnation more or less easy, the mode by which it has place, or the manner in which it acts*.

There is, perhaps, no phenomenon less understood than contagion, notwithstanding the labour which has been bestowed in the endeavour to offer some rational explanation of its origin, its nature, and its mode of action.

By some the contagion of variola has been compared to germination; and the supporters of that opinion assert, that a particle of the virus contained in a small pox pustule, when placed under the epidermis, produces a great number of similar pustules to that from which the virus is taken; like a grain of wheat, which, if thrown upon favourable ground, gives birth to a number of

* Rochoux.

similar grains. The advocates of this doctrine compare the production of typhus to the mode of action in putrid fermentation. Such comparisons throw no light upon the mysterious subject of contagion; and, whilst the operations to which this phenomenon has been compared are involved in the thickest obscurity, the comparisons can afford us no assistance. Before we can give any more lucid explanation of the nature of this principle, we must possess many data of which we are, at this moment, profoundly ignorant: we should first know the organic elements, whether solid or fluid, on which the contagious principle ordinarily exercises its most fatal power, &c.

Without such data, many points connected with the nature of contagion, and the mode by which it can be propagated, must still remain in obscurity.

If it be asked, why persons who have once suffered from certain contagious diseases should afterwards possess, to some extent, an immunity from a second visitation of the same disease, what rational or satisfactory answer can be given to such a question? If, again, it be asked, why certain contagious diseases require for their development an incubation (if I may be allowed the term) of many weeks?—what is the origin of the principle of contagion?—how is the virus of hydrophobia spontaneously produced?—we must confess that we do not at present possess the power of removing from either of those subjects the veil by which they are all obscured.

Whether or not infection and contagion be different principles, is a question equally difficult of solution.

It has been generally admitted, that the various principles of contagion must possess different specific powers; and, were this not the case, syphilis might produce hydrophobia, or itch the small pox. It is from our experience of the identity of all contagious diseases with that disease to which they owe their origin, that we deduce the existence of a diversity of principles of contagion; but, as contagious principles cannot be demonstrated by any modes of investigation with which we are at present acquainted, our opinion of their character can only be conjectural: and, until organic chemistry be more extensively and successfully cultivated than it has hitherto been, we shall not be able to discriminate between pus taken from a sluggish venereal ulcer and that obtained from a matured small pox pustule:

During a very long period of time, almost universal belief existed, that a contagious disease of the same kind could be produced only once in the same person. A contrary opinion is now entertained with respect to many contagious diseases; and the following relation must create doubts of its correctness in any.

In Egypt, a physician one day opened a bubo upon a person who had died of plague. An Arab being present, the physician said to him, may I put a little of this upon your arm? at the same time holding up the lancet covered with pus. The Arab hesitated, but said, what will you give me? What you will, was the answer. Two piastres? Yes; but mind, it will make you ill, perhaps kill you. The Arab, however, desiring to obtain the piastres, determined to brave the risk. The physician scratched the arm with the point of the lancet, took a little cotton impregnated with the pus, and fixed it upon the arm. Two hours had scarcely elapsed before plague was developed in the Arab. He was cured.

The physician thought this a favourable opportunity for determining whether the disease might be contracted a second time. The Arab submitted to a second experiment: a second time the plague is manifested, and a second time cured.

After these proofs, the Arab thinks himself secure; and the physician makes a third application; a third time it is followed by plague, and, singular as it may appear, a third time he is cured! At last he is subjected to a fourth experiment: a fourth time plague succeeds, and is followed by death.

The first victim did not satisfy this man: he repeated the experiment upon thirteen other persons. All suffered from plague. The lives of some were saved: others died. They were attacked at the end of one, two, three, four, five, or six hours; and the last, feeling very well at the completion of the fourteenth, was attacked at the fifteenth hour.

I confess I cannot contemplate this experimentum in anima viti without extreme horror.

The occurrence of cholera in the same person more than once is frequent. We may find, in the reports of Annesley, Jameson, Searle, Christie, and others, examples of the reappearance of the disease in districts which it has already ravaged, and of its reoccurrence in individuals who have before suffered from its attacks.

The word contagion, which is derived from the Latin word contagio, would imply, if literally translated, the necessity of contact. We admit, however, that contact may be mediate or immediate: the former effected through the medium of the atmosphere, or other means of indirect communication; the second by actual contact.

Those diseases propagated by actual contact are small pox, itch, hydrophobia, &c.

In this class, I apprehend, it is necessary for their transmission that a germ, which they appear to contain, be communicated to other persons by contact or inoculation. Thus, for the propagation of itch, no abrasion of the cuticle of the person to whom it is communicated is necessary; whilst, on the contrary, for the propagation of hydrophobia, actual inoculation appears to be indispensable.

These diseases really appear to be propagated almost in the same manner that organized beings are produced;

the germ being capable of reproducing and multiplying the disease.

The other class may be propagated by mediate contact, or indirect communication; and in this class I would place some fevers, as those of a typhoid character, in which, instead of a localized germ, we have a general exhalation from the mucous surfaces of the body, that, when sufficiently concentrated, or, in other words, when combined with atmospheric air in sufficient proportion, may communicate the disease to persons who, during a certain time, respire such air.

Those contagious diseases which are propagated by the medium of the atmosphere cannot, I apprehend, be so propagated unless the atmosphere be impregnated with a considerable proportion of the contagious principle, which, in this manner, becomes concentrated.

This concentration may be effected by crowding a large number of persons suffering from contagious diseases into confined and ill-ventilated rooms.

The disease is then communicated by respiration. The noxious air obtains access to the blood through the medium of the lungs; and we find, however early we prescribe blood-letting, even at the first manifestation of the symptoms of disease, that the blood presents a remarkably changed appearance.

Some years since, a very interesting sketch of contagion was given by Dupuytren, in which he maintains the same views I have laid down, and asserts that Nature resorts to diversified modes of communicating contagion.

If it were shewn that, in a very well ventilated room, contact, either by rubbing or otherwise, would communicate those diseases in which the germ is not localized, I must admit that the position taken by me would be endangered.

Whatever may be the modes of propagation in contagious diseases, whether by the atmosphere, by inoculation, or by simple contact, the principle is still preserved, "that contagious diseases are communicated according to one of these modes by persons suffering from those diseases to persons in health."

For the production of infectious diseases, it is again necessary that the infectious principle should be combined, in certain proportions, with the respired air. In a confined situation, then, cæteris paribus, a focus of infection will be more powerful than in the open air, where the diffusion of the infectious exhalation must ordinarily very much weaken its noxious power.

The principle of contagion or infection may be carried by currents of air to a distance from the place in which it is generated; but it must be recollected, that in an inverse ratio to its diffusion will be its power of producing disease.

The distance to which the principle may be carried, in a condition capable of producing or communicating disease, must be therefore dependent upon circumstances by which it is modified in a greater or less degree.

The absorption of the principle of infection may be succeeded by a contagious or non-contagious disease.

A circumstance presented by those contagious diseases which are contracted through the medium of respiration is this, that they commonly exhibit a typhoid character; a circumstance usually well marked and frequently observed.

In the latter stages of such diseases decomposition seems to occur; for, however soon after death the body is examined, decomposition is found to exist.

The tympanitic state of the abdomen, which is presented in the last stages of typhoid diseases, is clearly produced by the evolution of gas succeeding to decomposition.

A similar state may be produced in a dog by injecting putrid matter into his veins.

If this matter be diluted by agitating it with water, we find, that in proportion to the dilution of the matter, and the quantity injected, is the intensity of disease produced in the animal.

If, therefore, the same train of symptoms are produced by submitting the patient to the respiration of air rendered deleterious by the admixture of a substance succeeding to an incipient decomposition of an animal body, as by injecting putrid matter into the veins, we may fairly presume that such diseases are ordinarily produced by this matter getting access to the blood, and that through the medium of respiration.

Cæteris paribus, the intensity of character manifested by the disease is dependent upon the concentration of the principle by which it is excited.

Although in the preceding pages I have endeavoured to mark a broad distinction between contagion and infection, and although, to a certain extent, I may have been successful, yet it will be remarked, from what has just been said, that such distinction becomes extremely faint, and perhaps useless.

To shew clearly this circumstance, it is sufficient to consider, that we admit that a certain number of contagious diseases are propagated through the medium of the respiration of an atmosphere strongly impregnated by an emanation from the body of the patient.

It is true that much difficulty may be experienced in demonstrating the actual existence of a putrid decomposition commencing before death; yet perhaps few persons will be disposed to deny its existence under certain circumstances. If this be admitted, contagion and infection are, in this instance, scarcely distinguishable the one from the other, both proceeding from an exhalation from dead animal matter.

I do not mean to argue, that a similarity of disease would imply a similarity of cause or origin; but, if we assume, as I think we fairly may, that, in certain diseases, putrid decomposition does occur previous to death, how are we to establish the specific character of the emanations proceeding from such decomposition?

I have presumed that the blood is primarily affected upon the contraction of a contagious disease; and, if the presumption be a correct one, there is no difficulty in conceiving that many of the secretions from the blood may contain the principle, either in a gaseous or fluid state, which may be communicable to persons placed within its influence.

Collard de Martigny is convinced that the exhalations from mucous membranes, at least those of the skin and respiratory organs, are subordinate to the nature of the matter ingested. Does not this, then, strongly support the opinion that, supposing the blood to be affected by a contagious principle, its exhalations, or at least those from mucous membranes, may be endowed with the same principle?

It is certain that a gaseous exhalation from the skin is constantly produced, and that it is in an inverse ratio with cutaneous absorption.

It is almost constant and energetic in the morning and after an abstinence of twelve to fifteen hours. A law, then, governing the transpiration from mucous surfaces is, that it exists in an inverse ratio with absorption: if, then, absorption by these membranes is the mode by which a certain number of contagious diseases are produced, would not any mode, by which we could increase the function of transpiration at will, necessarily lessen the absorbing power of these membranes and the consequent absorption of contagious matter? If so, either by the ingestion of a large quantity of diluent fluids, or by injection into the veins, we might readily produce that effect.

If, in cholera, as has been stated, a few grains of nitrate of potash injected into the veins change the character of the blood, and render it fit for the purposes of life, why not, instead of resorting to an operation occasionally serious and always troublesome, mix a quantity of this substance in a sufficient quantity of fluid to be taken into the stomach? a mode this, little less speedy than the other, of carrying this salt into the blood.

Darwin states, that, having given a friend a few grains of nitrate of potash, in half an hour afterwards the salt was found in the urine.

CHAPTER IV.

PLAGUE.

THERE is much doubt whether simple contact will, under any circumstances, be sufficient to produce disease in a healthy person.

That point has been a fruitful source of discussion in plague, which, of all contagious diseases, has been most narrowly and carefully investigated.

Burdin says he has long been occupied with the important question of contagion, and expresses his regret that no positive experiments have been made, to set at rest the question, whether simple contact be sufficient for the purpose of propagating contagious diseases. Some years since, six young medical men volunteered to go out to Egypt for the purpose of submitting themselves to the influence of contagion in the neighbourhood of some place where any great contagious epidemic reigned. They proposed to wear the clothes of persons recently dead of plague, without previously submitting them to any disinfecting process.

It is to be regretted that the authorities in France, to whom the offer was made, did not countenance the heroic determination of these young men; for if the clothes taken from persons suffering from the disease had been hermetically sealed, and forwarded immediately to an uninfected district, and then worn by these young men, the question of mediate communication, in as far as plague is concerned, would have been decided. Of course, any similar experiment, made in a country where the disease reigns, must be inconclusive.

Burdin contends that the existence of contagion, even in plague, is not proved, so as to be regarded as an absolute certainty; and many eminent physicians deny its contagious property.

Assalini, one of the principal physicians in the French army which occupied Egypt, and Maclean, who has seen the plague in the Levant, state that it is not contagious.

D'Arcet, in a letter dated Tripoli, June 23, 1829, says, " the plague has not yet disappeared; but its habitual period of disappearance is near, and in three weeks it will have taken its departure."

With the contrariety of opinion which exists on the subject, it is strongly to be regretted that the Members of the Commission sent out by the authorities of France for the purpose of investigating the nature of the plague, especially in Egypt, had not resorted to unequivocal experiment for the purpose of investigating the nature of this disease. They should, at all events, have profited by the offer which was made to them by the Pacha of Syria. He placed at their disposal, for the purposes of experiment, men who were condemned to suffer death for crimes which they had committed.

During the time when the plague desolated Moscow, Catharine the Great placed at the disposal of the medical men, for the purposes of such experiment, a certain number of persons condemned to death. They wore the clothes of persons who had died of the plague; but, these clothes having been previously fumigated, and submitted to other modes of disinfection, the experiments were of no value: to have been conclusive, counter proofs should have been made.

There is another experiment equally required, to determine whether substances, immediately proceeding from persons affected with plague, and removed from the district in which plague exists to an uninfected place, can communicate the disease to other persons. If this experiment be not made, and repeated with much care, hypotheses must be still our guides, and absolute certainty will be wanting.

It must not be for a moment supposed, from what I have said, that I make it a reproach to those learned Commissioners that they did not remove the clothes of persons who had died of plague to a healthy district, for the purpose of performing an experiment which might be attended with such melancholy results. They would, by doing so, have incurred an awful responsibility. The facts which were collected in Egypt by these scientific and laborious men are most important; and if they do not establish, with demonstrative certainty, the contagious character of the plague, they afford the strongest presumtive evidence that plague may be, and often is, transmitted by contagion.

A man shut himself up at home during the existence of plague in a town, not suffering any one to approach him from without: a letter was brought and thrown into the house; he read it; in the evening of the same day plague was developed, and soon afterwards he died.

A person shut himself up, similarly isolated from all communication with his neighbours; but, for a moment forgetting himself, he took from the hands of a Turk papers of importance; the plague is immediately developed; he dies; and his family, consisting of eleven persons, who were all up to this period in perfect health, share the same fate.

The papers, the first apparent cause of ill to this family, were equally fatal to the Turk who brought them.

In these cases, we can explain the production of the disease only by one of two modes: either the letter had been subjected to miasma, had become impregnated with it, and it was conveyed to persons with whom it had contact, or an infected state of the atmosphere of the place developed the disease in this family at this period. The former, however, is perhaps the least strained conclusion; though, when we look to the country, where the disease so constantly exists, in an endemic and so frequently in an epidemic state, the latter opinion is any thing but improbable.

The neckcloth of a person who had died of plague was placed successively round the necks of six men; all of whom died of plague.

The dress of a person who had died of plague was worn successively by twenty-four persons, who all died of plague.

The experiments above cited were not conclusive; because the articles of clothes were not removed to uninfected places, and worn by persons who had no communication with the diseased district.

A man died in a town where the plague raged; his dress was conveyed to another town where the disease did not exist: seven children had communication with the dress, and all died.

This case is again inconclusive, in two respects; first, it is not shewn that the man who died in the town where the plague raged died of plague; nor that the disease of which the children died was plague.

That the plague is, however, transmitted by contagion, is now generally admitted.

Cases occur where we cannot conceive the existence of any focus of infection, except that furnished by human beings either suffering or dead from disease.

Up to 1815, the plague had not penetrated Hedjaz. In that year the troops sent from Egypt carried it to Gedda, which is the port of Mecca, and is a small town built upon the sands found on the borders of the Red Sea, and which are under the influence of a burning and unclouded sun. Here, the only probable causes of insalubrity are, the want of fresh water and the existence of excessive heat; but to these the inhabitants have been habituated from time immemorial. This town was almost entirely depopulated.

Occasionally, when the plague is raging, entire isolation preserves the person from the disease. This, however, proves little; for, without isolation, many escape: a remark which applies equally to cholera. Where the disease is endemic, isolation, however perfect, has been found to be useless; although it is stated, that, when the plague is imported into uninfected towns, and when the disease is aggravated neither by endemic nor epidemic causes, isolation is almost always a preservative.

If the Bedouins discover that the plague is near the desert, they fly; and should it have attacked any of them before, or make its appearance during, their flight, they leave the sufferers behind them, with some food, under a tent, and continue their flight. Thus they soon escape the disease; and this, it is submitted, furnishes further evidence of the necessity that, in contagious diseases, the principle should be concentrated, to ensure transmission. Contagion alone would, however, I submit, be sufficient to produce the ravages which the plague commits.

If, in Egypt, the birth place of this horrible scourge of the human race, I look at the time when it is manifested with most violence, I cannot help feeling strongly impressed with the conviction that it is primarily dependent upon exhalations proceeding from the action of the sun upon the alluvion deposited at the overflowing of the Nile, and that to this cause we must attribute the fearful epidemics of which that ill-fated country has become the prey.

When such a principle of infection exists, its effects are first felt by those least able to resist disease; and with every succeeding death the power of the principle is multiplied, by the creation of a fresh focus of decomposition. When the exhalations from the alluvion cease, the disease loses its intensity, and becomes for a time almost extinct; a circumstance which, I submit, tends strongly to prove that, whatever part contagion may have in propagating the plague, the extensive mortality of that disease, nay, even its primary existence, depends upon some principle distinct from and more powerful than contagion.

All, then, that we know of this disease is, that inoculation, or the contact of infected clothes, is sufficient to occasion the disease; but it is not so evident that it is propagated through the medium of respiration.

CHAPTER V.

CHOLERA.

I SHALL now examine, as far as the data of which we are in possession will enable me, whether the cholera which has ravaged a large portion of the old world has been propagated by infection, dependent on local exhalations or general atmospherical influence, or by contagion, transmitting the disease from person to person, or by all these causes in conjunction.

I have already detailed, in the chapter on Infection, the sudden invasion, the fatal character, and the abrupt termination, of the cholera which broke out at the source of the Ganges in 1783; the appearance of cholera amongst the detachment of Seapoys which, in the month of May 1817, encamped on the border of a lake near Sangar, and the recovery of those men who were able to leave the encampment; and also the appearance of the disease in the crew of the Fils de France a few days after her arrival in the Ganges: all instances in which infectious exhalations were almost demonstrable. Cholera has then, as I submit, been produced by local exhalations, and may therefore owe its existence, at least, to an infectious principle.

In many instances, cholera in India has followed exactly the marches of bodies of troops; of which we find many examples in the documents of the public boards of that country.

On the 22d of January 1822, the Cleopatra frigate cast anchor in the Manilla Roads, the cholera then existing on shore.

When the vessel cast anchor, the crew were in perfect health; but on the 30th, the men having communicated with the shore for the purpose of procuring water, the cholera was manifested on board; and the number of men attacked became in a few days so great, that, on the 7th of February, the commander of the frigate thought it prudent to leave the place.

The cholera existed at Orenbourg in the month of September 1829.

A resident at Rassipnoe, a village on the Russian frontiers, having business to transact at Orenbourg, proceeded to that town on the 23rd of that month, being then in perfect health.

On his return home, he was unwell, and died the next day of cholera; which immediately manifested itself at Rassipnoe, where 305 persons were attacked by the disease.

In the month of September 1818, the Topaz frigate sailed from Ceylon, where the disease then raged. Free communication had been permitted between the crew and the shore, and at the commencement of the voyage many individuals on board were attacked by the disease.

The vessel proceeded to the Mauritius, where she arrived on the 20th of November, and where free communication with the shore was permitted; the disease soon broke out on the island, and 6,000 persons perished from its ravages in six weeks.

Upon the arrival of intelligence at the Isle of Bourbon of the occurrence of the disease in the Isle of France, the governor took immediate and rigorous measures to intercept all communication with the latter place.

In spite of all precautions (and it will ever be so), a boat from the Isle of Bourbon proceeded clandestinely, on the 8th or 9th of January, to a small vessel belonging to the Isle of France, and which had left Port Louis on the 6th.

On the second or third day after this occurrence, the disease was manifested in the Isle of Bourbon; upon which, the governor immediately resorted to such measures as he thought the exigency required.

For the purpose of preventing the propagation of the disease, he established cordons, by means of which he isolated the disease. The sick were immediately removed to a lazaretto, and the disease was arrested; while it was still raging in the adjacent island.

The gates of Ispahan were closed against a caravan, containing pilgrims, some of them affected with cholera; the caravan proceeded to Jezd, where it obtained admission. The disease soon afterwards broke out Jezd, and numbered in that town 7,000 victims; while Ispahan at that period escaped the disease.

M. Francais communicated to the Académie Royale de Médicine, at Paris, the substance of a letter, dated Vienna, September the 1st, 1831, which stated that the sanitary cordon of Gallicia having been removed, by the advice of Dr. Stiff, Physician to the Emperor of Austria, who considered cholera as non-contagious, the disease penetrated into Hungary, where it had not before existed, and advanced towards Vienna; having, between the 18th of August, when the cordon was removed, and the 1st of September, when the letter was written, manifested itself in 200 Hungarian villages, and attacked 19,338 persons, of whom 10,000 died, 3,000 were cured, and the remaining individuals were still suffering from the disease.

In Russia, it is certain that, in very many instances, the cholera did not attack exclusively a great number of persons at the same time, as has been observed by physicians who have watched the disease in India: that individuals were attacked successively the one after the other, and that under circumstances where the patients inhabited small, ill-ventilated places, or where relations, friends, or attendants, remained too long or in too great numbers round the patient, cholera has been communicated to them; and that, at Moscow in particular, those persons in immediate attendance upon the sick suffered in a much greater proportion than the other inhabitants of the town.

It appears to be established, that the cholera was introduced into Sunderland from Riga.

I learn that the cholera has been traced, in Sunderland, to a woman who washed for seamen frequenting that port, and that she was the first person attacked by the disease in that town.

She had, shortly before, washed the clothes of the master of a merchant vessel from Riga. Whilst at Riga, he had been attacked by the disease, and the clothes washed for him at Sunderland were those he wore whilst suffering from the disease at Riga.

Can any similar facts be more clearly demonstrated than the introduction of cholera by means of contagion at the Isles of France and Bourbon, and at Sunderland? Upon the faith of such evidence, I submit that I am perfectly justified in stating, that cholera is, under certain circumstances (which have been already described), contagious, and is transmitted from person to person.

In adopting, as I have done, the opinion that cholera becomes, under certain circumstances, contagious, and is transmitted from person to person, I have been uninfluenced by any early prepossession in favour of that theory.

My original opinions were opposed to the possibility of the transmission of the disease from person to person; and I am not insensible of the difficulty of reconciling the contagious character of cholera to any of the physiological or pathological laws with which we are at present acquainted. But we are called upon to study and describe a disease which, raging as an extended and murderous epidemic, does not appear to be arrested by atmospherical vicissitudes, changes of season, or local peculiarities—which is presented to us independent of all special conditions that can be discovered by our senses, and of all local circumstances or hygienic influences known to destroy vitality—which appears sometimes to be propagated by means of occult causes that we can neither appreciate nor know, and of physical individualities that we can neither describe nor discover—and which we can therefore no more subject to general laws, or explain by scientific rules, than we can tell why the eye sees or the ear hears, why the blood circulates in the system, or how respiration preserves life.

In order that a disease shall be contagious, we require, as has been already stated, that it shall produce a principle by which it may become transmissible.

According to the experiments I have detailed in the chapter on Plague, this principle may be either an exhalation from the surface of the body, or the pus secreted in a bubo: the one requiring for its transmission a contact, prolonged during a certain period; the other requiring abrasion of the skin.

In cholera, we have no secretion of pus, by which we can inoculate an individual; and it does not appear that the secretions from the skin can produce the disease; at least, the weight of evidence is opposed to this opinion.

If, then, we admit the existence of contagion in cholera, by what mode can it be produced, except through the medium of pulmonary exhalation and absorption?

In ill-ventilated situations, and in apartments where individuals have been crowded together (other circumstances being alike), the disease has always proved most destructive; whilst, in proportion as we promote cleanliness and ventilation, the power of propagation diminishes. Does not this strongly support the conclusion, that the disease, even when contagious, is communicated by the atmosphere? Is not the action of the principle of contagion thus assimilated to that of infection?

At Newcastle, the disease has been entirely restricted to those situations in which filth and misery of every kind are almost exclusively found.

During the festivities of Christmas, when a great number of the poorer classes are accustomed to surrender themselves to every kind of dissipation, a large number of that class of the inhabitants of Gateshead resorted to Newcastle, and more particularly to the infected portions of that town; they returned to their homes, and the following day was ushered in by the appearance of cholera, which immediately manifested extreme intensity.

The greater violence which for a time characterized the disease at Gateshead, as compared to Newcastle, is easily explicable, by the circumstance, that the former place is the residence of a greater number of persons who are generally subjected to great privations, and who are crowded in numbers into miserable, confined hovels, in which any ventilation is almost impracticable.

It is simply, I submit, from the superior ventilation and cleanliness in the houses of the better classes, that they have almost a perfect immunity from the disease. It is from this reason that, at Cronstadt, where 1,800 persons were attacked by the disease, only 14 were above the class of serfs.

The simultaneous appearance (if it really be so) of cholera in entire districts far removed from each other, the sudden violence with which it is manifested, the great number of individuals who have been attacked at the same time, the rapid progress of the disease, and its sudden disappearance after great ravages, are all considerations which must arrest the conclusion, that these ravages are entirely dependent either on local exhalations, on contagion, or on both.

Through the greater part of India, the persons in attendance upon the sick suffered in a less proportion than others. Persons suffering from cholera, in India, have been placed in well-ventilated wards, in the midst of patients suffering from other diseases, and have not communicated the cholera to the other patients. When a person suffering from cholera remained in his house, the other members of the family did not usually suffer from the disease in a greater proportion than the rest of the population of the place. When there have been more persons than one ill of cholera in a house, they have been frequently attacked in so sudden a manner, and so independently the one of the other, that it is difficult to suppose that the disease had passed from one to the other.

If we look at the disease at Port Louis, in the Isle of France, we find it stated, that it appeared suddenly, and almost simultaneously in the different quarters of the town, and that negresses, who, during many days, were in constant communication with the vessel, were not attacked by the disease.

The disease did not appear either more promptly or with more violence in the environs of the place where the Topaz frigate was anchored than elsewhere; and the medical men who observed its progress state, most positively, that it attacked a great number of individuals who had nothing else in common than the air which they respired.

The invasion of the disease generally occurs either at night or towards the morning.

In some places and under some circumstances, individuals, some knowingly and others ignorantly, have lain upon the beds which had been just left by cholera patients, some of whom had been cured and others had died of the disease. They have worn their shirts and other articles of their dress; they have respired the air exhaled from the mouths of patients; they have passed the night in the same bed with patients; they have rubbed their skin with the perspiration discharged from patients; and, under all these circumstances, escaped cholera. These circumstances have, however, taken place in only a few instances; and, although they may, to a certain extent, shew that the skin is not a medium by which cholera can be communicated or received, yet, as we have no information as to the state of ventilation where they occurred, they do not shew that cholera cannot be communicated by means of respiration in ill-ventilated situations.

Cholera has not only been preceded by epizootia, more or less violent, from which different species of animals are said to have suffered, but, during the existence of the disease, persons have experienced disorders simulating to, though not identical with, cholera. These circumstances, however, have not been generally observed, and may therefore be fairly referrible to local conditions.

Cholera appeared simultaneously and suddenly at Bahar and at Decca, two towns in India, distant 120 leagues from each other.

Two other towns, Nagpore and Moltay, were dreadfully afflicted by the disease; whilst all the intermediate country remained healthy. Still we have here no evidence to shew that the disease was not imported into each of these places simultaneously. Indeed, it would appear most probable that such was the case.

The individual invasion of the disease is occasionally so sudden, its character so violent, the time which occurs between perfect health and the perfect development of the disease is so short, that it becomes impossible to catch any traces of intermediate change, such as is constantly found to exist in the development of diseases which are transmissible by absorption.

Patients are, it is said, frequently affected without having approached any person suffering from disease; and really, in these cases, we can assign no other explanation for the epidemic than some suddenly developed atmospherical alteration—frequently, however, insensible.

An elevated temperature, comparative, however, with reference to the climate of each country, has always been esteemed a condition of the existence and propagation of all contagions, and more particularly of those of exotic origin.

Cholera is developed and preserves its character in a temperature of 90 degrees above zero in India, and of 30 degrees below zero in Russia.

Irresistible in its progress, cholera, in a comparatively short time, extended itself over the greater part of India, comprising large tracts of country presenting nothing in common except the atmosphere by which they were surrounded.

In the middle of a vast country, suffering from the cholera in all its fury, we find bands of country, of considerable space, where the disease has not penetrated, though the environs were the seat of despair and destruction.

I apprehend that we may fairly explain some of these circumstances by assuming, as I think we are perfectly justified in doing, that the cholera in India, and the plague in Egypt, are developed under circumstances almost similar the one to the other; that both owe their origin to infectious exhalations; and that, although their propagation afterwards may be by means of contagion, yet they require, for this purpose, the concurrence of circumstances which I have already named.

In explaining the propagation of cholera, Shnurrer says, we can seldom refer its appearance to the direct communication of one individual with another. Sometimes it leaps over many points in the line of its march, and afterwards returns with much violence to the place first spared. Its propagation has no sensible relation to the variations of temperature. In the early part of 1819, when it appeared at Ceylon, it was manifested also more eastward, at Aracan, Malacca, Sinkapore, and Java. An attempt has been made to shew that an intimate connection existed in Java between the disease and the volcanic eruptions by which it was preceded; and again, that the disease has been suddenly arrested in its progress at a time when volcanic explosions have been experienced. At Manilla, the disease was manifested in three days after a tremendous hurricane.

In June 1822, it was first observed at Mossoul, in October at Orfa, in November, almost at the same moment, at Ber, Aintab, and Aleppo; and in each place, but more especially at Aleppo, it was preceded by earthquakes.

At the end of August 1821, it broke out at Bagdad, and was especially fatal at Schiraz; and, in each of these cases, was preceded by earthquakes.

Some persons, again, have endeavoured to refer the production and termination of the disease to volcanic or electrical agency; and that simply because, on the occasions to which I have referred, tremblings of the earth were noticed a short time before the manifestation or termination of the disease. When we reflect upon the frequent occurrence of such convulsions in the Indian Archipelago and in some portions of the continent of India, we shall not be surprised that the manifestations or terminations of the disease have, on some occasions, been coincident with these convulsions.

In the vast majority of cases, no such coincidence has been observed; and, even on those occasions where subterraneous agency was coincident with the development of cholera, the disease presented no peculiarity which did not accompany its ravages in places where no volcanic agency preceded or accompanied its propagation.

Other persons have sought to explain the origin and propagation of cholera, by stating, that it resides in a change of relation between the atmospherical and the animal electricity. I am not prepared to say that this doctrine is incorrect; but it certainly is a gratuitous assumption, entirely unsupported by evidence.

It has also been stated by Dr Tytler, that at Jessore, which is a hundred miles north-east of Calcutta, in the delta of the Ganges (a place to which, as I have already stated, the cholera owes its origin), the development of the disease was attributed to the bad quality of the rice, which is the general food of the Indians. Whether this may or may not have been the origin of the cholera, is now an object of minor interest with us: the important question is, how is the disease propagated? and not how did it originate?

If we look over the extent of country which has been traversed by cholera, we must be convinced that foci of infection did not exist in all the places where it has appeared. We have shewn that the activity of putrefaction is in a direct ratio with heat; that the immense marshes in the north of Europe, of Russia, of Lithuania, have scarcely any influence on the health of persons residing in their vicinity; that, in temperate countries, in spring, marshes communicate to the atmosphere only humidity, and that, after autumn, the fevers which are caused by the exhalations proceeding from decomposition in these foci cease to be renewed; that the marshes of equatorial regions are much more dangerous (to this Western Africa owes its character of being the most unhealthy portion of the globe); that the insalubrity is great at Guyana, Bengal, Batavia, and other tropical countries; that all marshes and other sources of putrefaction exhale powerful odours, and excite much disease in summer and autumn; and that, during the existence of the south wind, miasmatic diseases are increased in number and intensity.

But, if we look at the traces of cholera, how few sources of such emanations can be discovered in the line of its march!

At Orenbourg, which was the first part of Europe attacked by cholera, there were no marshes; but we find running through the valley in which it is built two rivers, the Sakmare and the Oural. The soil too is argillaceous, sandy, and habitually dry. Here, therefore, we have no source for the generation of infection.

Cholera, as we admit, may be developed under the influence of an infected atmosphere; but infection cannot enable us to explain how cholera has been extended over vast tracts of country, or how it has been developed in individual towns or districts where no form of infection can be discovered.

Diseases dependent upon infectious exhalations are generally of a typhoid character; epidemic cholera is not so.

In cholera, the patient preserves, almost to the last moment, his intellectual character. The consciousness, the judgment, the reason, the reflecting power, are all unaffected; and we do not observe in epidemic cholera the numerous symptoms of vital reaction inseparable from typhus, as heat of the skin, redness and animation of the countenance, or cough: neither in cholera do we see supervene exanthemata, sudamina, petechia, &c. In the face of such discordant results, furnished by men whose powers of observation are undeniable, to what conclusion can we come?

Is there a possibility of reconciling, in any manner, facts and conclusions so diametrically opposed the one to the other? When we see that cholera has existed among numerous troops, and that change of place and subdivision alone have been constantly found either to terminate or mitigate the power of the disease—that crowds of persons leave a town in which it exists—that some of them are affected by the disease, in stages more or less advanced—that the progress of the disease shall be arrested, those persons who have already suffered becoming frequently convalescent, and those who have not been attacked escaping the disease altogether—that it has attacked towns and districts far removed from each other, almost simultaneously, while the intermediate country has enjoyed the most complete immunity—that, instead of increasing daily in a place which it has attacked, as if feeding upon the increased nutriment which a contagious principle would afford to it, the disease has invariably been found to exhibit a regular progress from increase to maturity and decline to extinction—that the duration of the disease is circumscribed within narrow limits, varying from three weeks to three months, the latter, however, being a period that has seldom occurred in India—that, in the first period of the epidemic, the disease is rapid and generally mortal; in the second period, less rapid in its progress and less mortal in its effects; whilst, still later, it becomes yet less formidable, and the deaths more rare—that, according to a calculation made by Dellon, the proportion of deaths, in the earlier stages of these epidemics, was as nine to twenty-four, and, towards the decline, as one in fifteen—that, looking again to the army, where, from the greater facility with which we obtain information, our data are generally more correct, we find that, in the centre of the English army, cholera appeared upon the 7th of November; that, from the 16th to the 22nd, it acquired its greatest degree of intensity; that it had much diminished by the end of the month, and had completely disappeared by the 2nd or 3rd of December; that, in the left division, the disease appeared on the 10th of April; had, by the middle of the month, attained its greatest vigour; by the 21st had lost much of its intensity; whilst, at the beginning of May, not a case remained—that, among other troops, the progress was still more rapid; at Rajapoutana it appeared on the 14th of September, raging with fury until the 17th, and then rapidly subsiding; so that, on the 1st of October, it had entirely ceased—that bodies of troops have been attacked by the disease, and have joined other bodies, which, in spite of this union, have preserved their health—that, notwithstanding the sanitary laws, especially that of cordons, were enforced at Moscow, as well as at Berlin and St. Petersburg, they were entirely inefficacious to prevent the introduction of the disease into those places; and that Thorn, without any precaution, and in constant communication with the diseased districts, escaped during so long a period; while the disease manifested itself earlier at Dantzic, which is near the mouth of the Vistula—

when, I repeat, we attend to these facts, we must be satisfied, whether cholera be not, as I submit it is, under certain circumstances, a contagious disease, or, as it is contended by many individuals, non-contagious under any—its rapid propagation and frightful mortality, extending, as they have done, over much of the old world, under every diversity of temperature, must have been produced by the combination of other means with contagion.

I come, therefore, to the conclusion—first, that cholera originated in local exhalations dependent on animal or vegetable decomposition; secondly, that the disease has produced a contagious principle, and has been transmitted from person to person by the respiration of air to which that principle has been communicated in sufficient proportion; and, thirdly, that as well local exhalations as the principle of contagion may be transmitted to a distance by the medium of the atmosphere, and that the extension and frequently rapid diffusion of cholera, and its almost simultaneous appearance in distant districts, are, with our present information and knowledge, explicable only by atmospherical influence.

CHAPTER VI.

REMEDIES.

THE question now presents itself, can the principle of infection or contagion, however primarily originated, be neutralised or destroyed? and if not, can we place ourselves beyond its influence?

Experience justifies our asserting, that particular states of organization in an individual (from which results his physical power, and the more or less perfect regularity with which his functions are performed) may oppose themselves to the reception of disease.

Experience also justifies us in believing, that what is termed a good constitution is less accessible to disease than a bad one. We only know a good or bad constitution by its effects.

If a person is frequently ill, although placed under conditions favorable for the preservation of health, we may fairly conclude he has a bad constitution; if, on the contrary, he resists those causes which produce disease in others, we may justly say that he has a good one.

Can any mode be resorted to, with success, for preserving an individual from the effects of infection or contagion? and if we are acquainted with any such influence, does it possess uniformity of action, or is it necessary that it should be varied in different diseases?

As we admit that the principles of contagion differ, the one from the other, and as we may assume that they also differ from the principle of infection, we conclude that the means which should be opposed to their reception into the system and their transmission from person to person should also be varied.

In certain contagious diseases, cauterisation is stated to be a powerful agent for preventing the development of the poison; but only in those where the virus is applied to a limited point. In hydrophobia, it is believed that, by cauterising with promptitude and energy the bitten portion, we shall neutralise the virus deposited in the wound by the teeth of a rabid animal. In itch and syphilis, we know that if we refrain from mediate or immediate contact with an affected individual, we run no risk of contracting the disease.

In small pox we possess an agent by which a barrier (though not an impenetrable one) may be interposed to the passage of the disease from a person suffering from the disease to another person in health; and, in contagious diseases, propagated through the medium of the atmosphere, we know that there is a distance beyond which they are not communicable. There are then certain principles which govern the propagation and action of contagious diseases, with the laws of which we are acquainted; a knowledge this by which we may be enabled occasionally to prevent the communication, and, even when communicated, to arrest the progress of the disease.

Our first duty, then, should be to ascertain with exactitude what are the sources from which the disease has originated, and afterwards to put in practice such methods of prevention or cure as may be within our reach.

It is by resolving in different modes this difficult problem, that the contagionists and anti-contagionists are engaged in an animated struggle.

Supposing then that, after cautious and deliberate enquiry, we have satisfied ourselves of the infectious or contagious character of any disease communicated by the atmosphere, as well as of the source whence this infection or contagion proceeds, what measures should be resorted to for the purpose of preventing propagation?

1st. If noxious exhalations exist, we must, if practicable, destroy or correct them ; if impracticable, and their extent be limited, we must remove ourselves beyond their reach.

2ndly. We must, as much as may be practicable, isolate the sick from the healthy.

3rdly. We should secure a perfect ventilation of the apartments, as well of the healthy as of the sick.

I assume that the same method should be resorted to for destroying or neutralising the effects of both contagion and infection, and I therefore treat of them here conjointly.

Supposing that the source from whence this principle is derived be so extensive as to render abortive any attempt to destroy it on the spot, and that this source be an immense number of individuals crowded into narrow spaces, we may, by separating the patients and removing them to well-ventilated apartments, effectually neutralise or mitigate that principle. In such a case isolation is unquestionably one of the most effective measures to which we can have recourse.

If it be not established, as a principle, that typhus, yellow fever, plague, or cholera, may communicate a similar disease to persons who approach them, and by this means to a whole town or country, assuredly, until their non-contagious character has been certainly demonstrated, the strictest and most entire sequestration should be placed upon persons suffering from these diseases. I, however, again repeat (as a broad principle) that, in all those diseases where the principle is not localised, that is to say, all diseases possessing a typhoid character, it is necessary, for the purpose of generating the principle of the disease, that many persons be crowded into narrow spaces, and that the respired air be highly charged or strongly impregnated with this principle. Resting upon that conviction, I unhesitatingly state, that, if in these diseases patients are not crowded into narrow spaces, but are, on the contrary, placed in well-ventilated rooms, no apprehension need be entertained of the communication of the disease, even to persons that are in attendance upon the sick.

Isolation, to be useful, then, requires, as a primary condition, that patients be not only isolated from healthy individuals, but also from one another, and that they be placed in a situation where the air is pure and frequently renewed.

In diseases caused by infection, if we cannot remove the infectious principle from the inhabitants, the utility of removing them from the principle must be admitted. Numerous observations attest that change of place presents a very efficacious means either for escaping or curing the disease.

Dupuytren, in his report upon the yellow fever, says, "If there be a single fact demonstrated by reason and by experience, it is that the sojourn in a place where this disease exists is essentially pernicious; that the cause and the effect of this disease, multiplied the one by the other, acquire a frightful activity; that all measures concerted for the purpose of destroying them at the place are insufficient; and that we should attribute to that obstinacy which retains citizens in an infected town, the ravages of the disease, which, according to the time, the place, and the circumstances, may destroy a tenth, a ninth, a sixth, and, horrible as it may appear, a third, and even half the population."

If, however, the necessity be admitted for removing the population from a place in which an epidemic reigns, it must be obvious that much circumspection should be observed in the selection of the places to which they are taken; for it is quite possible they may be localised in a situation of doubtful salubrity, that may present similar conditions to the place from which they have been removed.

The value of isolation is established in the following case:

In 1822, at the time cholera approached Aleppo, M. de Lesseps, the French Consul at that place, with all those who chose to accompany him, took refuge in a garden at some distance from the town. This asylum was enclosed by walls and surrounded by a large ditch; there were only two gates, one for entrance, the other for escape.

During the existence of the disease, he admitted nothing from without until it had been submitted to the precautions in use at lazarrettos.

This colony of two hundred persons, composed not only of French but of the natives of the country, was preserved from the disease; while, in the town, four thousand persons were destroyed in eighteen days.

Cordons have been long employed, during the existence of contagious diseases, as means of isolation; and until recent times no voice was raised against their use, however rigidly they may have been enforced: but in the present day an opinion is generally entertained that they possess at most only a doubtful efficacy. The relation which, in the last chapter, I have given of the introduction of the disease into Hungary, on the removal of the cordon established between that country and Gallicia, serves to show the importance of sanitary cordons under rational and prudent regulations. Teheran was preserved from cholera by the sanitary regulations adopted by the Persian Emperor, at the suggestion of Martinengo, an Italian physician. In many other instances, districts, towns, and fortresses have adopted rigorous sanitary measures, and the cholera has not been manifested. It is true that the cholera has made its appearance in despite of the employment of the most rigorous sanitary measures. This is at best only negative evidence, and cannot be opposed to the affirmative testimony establishing the importance of isolation.

The disease may have been in those cases dependent on local infection, or propagated by atmospherical influence, or may have been introduced into the town, notwithstanding the establishment of cordons, by the admission of individuals or the introduction of goods. At Aleppo, the case of M. Lesseps almost demonstrates that the disease was propagated by contagion alone; if it had been dependent upon atmospherical conditions, produced by subterranean agency, by local exhalation, or other means, that colony were clearly equally subjected to their action.

It is a work of extreme difficulty to prevent every kind of communication; and, when accomplished, the interruption of commerce, the want of employment, and the consequent distress, may so aid in the propagation of the disease as to occasion greater evils than would be occasioned by a communication almost unrestricted.

If recourse be had to cordons, they should be established under certain restrictions; they should be placed at the greatest possible distance (compatible with entire isolation) from the seat of disease, leaving between them and the infected place sufficient space for the persons who inhabit such a place to leave it, and yet find within the circle described by the cordon healthy habitations and salubrious situations for exercise. Under opposite circumstances, this means, instead of preventing the extension of contagion, will be productive of the most fatal consequences; because it will oblige the unfortunate inhabitants of a town which has become a prey to disease to remain in the centre of infection or contagion, inhaling a poisoned atmosphere, and depressed by the alarm, the terror, and the other passions which are excited by the presence of a murderous epidemic.

During a long course of years, the belief has existed that certain species of clothing possessed the power of preventing the communication or reception of contagious diseases. That opinion is in the present day exploded; and if the position be a correct one, that, in those diseases in which the germ is not localised, respiration of the miasmata is a sufficient and indeed the only probable means of communicating the disease, clothes of any kind can afford us no protection. The only way in which their pretended efficacy is explicable is by assuming that these diseases may be absorbed by simple contact with the skin, and that clothes present a mechanical obstacle to the action of this principle—a protection which is afforded by all clothes alike.

Have we any method of neutralising the effects of the principle of infection when its existence is well established? If the disease be produced by some focus of infection, dependent on the existence of putrid exhalation of limited extent, we should then with confidence fumigate, by disengaging chlorine from any substance with which it may be combined, and suffering the atmosphere of the place to be impregnated with this substance; or we may employ another class of powerful disinfectants in a fluid state, the chlorides of soda and lime.

With regard to these agents there are differences of opinion. Pariset is strongly impressed with the belief that they are universal disinfectants; and has broadly stated his conviction, that by their employment the plague might be dissipated from the soil of Egypt.

The experiments which were made by the commissioners who were desired to report upon the disinfecting power of these agents were any thing but conclusive. Six men's dresses were purchased which had been taken from persons who had died of the plague. These clothes, after having been plunged during sixteen hours in a solution of chloride of soda, were washed, and when dry were worn by the commissioners, without communicating to either of them the disease.

These experiments, to be worthy of confidence, ought to shew, that, if they had been worn without this precaution, they would have propagated the disease.

The Consul General of France, at Aleppo, reported to his government upon the action of chlorine during the existence of the plague in 1827, which destroyed twenty-five thousand of the inhabitants of Aleppo.

Two of his Janissaries, who, from the nature of their occupation, were in constant communication with persons suffering from the plague, and constantly present in the situations in which it raged, and who had suffered in their own families—one the loss of a brother and daughter, and the other of a wife and sons—were provided each day with the chlorides mixed with water, and this was used by them in the presence of the Consul; both were preserved in a state of health. The Janissaries of other Consuls were not so provided, and they suffered proportionably with the other inhabitants. Every one applied to the French Consul for some of the preparation, the effects whereof had already excited universal attention; but the small quantity which he had (barely sufficient for himself) prevented him from acceding to such requests.

He, however, at the urgent request of the Austrian Consul, sent him a small quantity of the chlorides, for the purpose of being used by two employés, who, under an imperative order, were obliged to visit some merchandise which was known to be infected: they performed their perilous task without accident.

After having mentioned many other similar experiments, he says, "Without pretending that my opinion ought to decide that these preparations are a universal preservative against plague, I may declare that all the experiments touching the preservative power of these substances have been made with the utmost circumspection, and have been attended with the greatest success."

If it be sufficiently proved that the clothes which have been subjected to disinfection, by means of the alkaline chlorides, were really impregnated with miasmata capable of producing plague (and of this there is perhaps no legitimate reason for doubt), then it is equally evident that we have, in these disinfecting substances, an agent by which these miasmata may be neutralised or destroyed.

Whenever it is established that an epidemic disease is dependent upon the influence of infection, and this infection does not proceed from a source of limited extent, it may be laid down, as a general rule, that all our disinfecting resources will be unavailable.

When the air of a town is vitiated by the emanations which proceed from any focus of infection, all our means of disinfection will be useless, unless applied to the source from whence the infection proceeds.

Our means of disinfection do not become valuable until applied to a circumscribed focus of infection, as a church, a hospital, a house. It is in such cases that we may act with the greatest advantage.

If the infectious principle be attached to clothes, or other similar substances, we find still one and the same cause, unsamata, susceptible of producing it; and the disinfecting agent ought then to be the same.

A French vessel, belonging to Bordeaux, which was detained for months at Calcutta, surrounded by other vessels, of which the crews were suffering from cholera, was provided with chloride of lime, with which the deck and cordage of the vessel were sprinkled; and the crew remained in perfect health.

Due and proper attention to ventilation and cleanliness will certainly do much to neutralise the poisoned atmosphere, whether dependent on local exhalation or contagion.

When these have been attended to, patients have been visited, touched, cleaned, dressed, and removed, without communicating the disease; and medical men have examined the bodies of patients with immunity.

It is to the attention paid to these circumstances that we should, in all probability, attribute the immunity enjoyed by the surgeons who attended the army in India; and to the neglect of them, the mortality amongst the hospital attendants in Russia.

Cholera, like all other typhoid diseases, selects, as its victims, the inhabitants of damp situations, and of close, confined streets. Poverty, squalid misery, debauchery, want, and mental anxiety, are its certain allies; and we shall most successfully wrestle with the disease by promoting the cleanliness, correcting the habits, and increasing the comforts of our poorer neighbours.

We thus adopt the course most likely to avert the presence of the disease (of which, however, I have little hope); and we shall certainly meet it ourselves, and enable our poorer countrymen to do so, with physical constitutions and moral dispositions best calculated to resist its influence.

The facts bearing upon Epidemia, Infection, and Contagion, have been now narrated, and the observations and reflections to which those facts give rise have been submitted to the public; the task of the Author is therefore done: but, before he parts with those readers who may have accompanied him through the preceding pages, and whom he may never again have the good fortune to interest, he asks for the usual privilege of leave-taking.

If the Essay now offered to the world possesses any merit, that merit will be due to a laborious collection and faithful narration of facts bearing upon the questions of infection and contagion—questions always of vital importance, and possessing, at the present moment, an absorbing interest.

It is too often the fate of scientific questions to be discussed more with the view to support a particular hypothesis than to elicit truth; and so long as plausible theory and positive and dogmatic assertion shall be received with more favour and rewarded with more liberality than persevering industry and patient investigation, so long will rash and visionary speculations take place of cautious discrimination and hesitating decision.

With a large part of mankind, oracular assertion and confident assumption are synonymous with talent and acquirement; and the slightest hesitation in pronouncing a positive opinion, on an abstruse and difficult subject, a certain sign of the most deplorable ignorance.

The nature, the origin, the action, and the propagation of cholera, are, each and all of them, subjects that now divide the world, medical as well as non-medical, into two great parties. Every man is a believer in the doctrine of contagion or non-contagion; and which will hereafter be the orthodox faith, there may now be considerable difficulty in determining.

Each doctrine is, perhaps, supported with equal skill and talent; the facts by which each is sought to be established seem pertinent and unanswerable; the cases cited by one party, perhaps, calculated to overthrow the doctrine of the other. Under these circumstances, may we not be permitted to doubt whether the disease originates in all cases from the same causes? and whether it may not be attributable, in one instance, to atmospheric influence, in another to local exhalations, and in a third to transmission from person to person by contact, either direct or remote?

We have shewn the disease attacking, almost simultaneously, whole districts; sudden in its approach, evanescent in its duration, and powerfully modified by changes of temperature, whether from cold to heat, or heat to cold. If these data be correct, and there were no possibility of transmission from person to person, or foci of infection, can we feel any hesitation in terming such a disease atmospherical?

We have shewn the disease breaking out suddenly in particular situations; where, on enquiring, we readily detect exhalations proceeding from animal or vegetable decompositions: its progress is rapid; its ravages are frightful; its victims numerous: but it is circumscribed within definite limits; and no contact with the sufferers when beyond those limits appears to produce the disease. What is this but infection?

We again find the disease appears in districts where no predisposing cause can be discovered: we learn, on enquiry, that persons have been admitted or goods imported from a diseased district; that persons, otherwise healthy, on being placed in contact with the sick, have become diseased; that those persons have proceeded into other districts perfectly healthy until their arrival, and that the same disease of which they have suffered, or are suffering, has broken out there. What is this but contagion?

If therefore cholera be generated by the atmosphere (which I can scarcely believe), be produced by local infection, and be transmitted from person to person by contagion, how are we to guard against this dreaded, this dreadful scourge? We must, as far as in us lies, correct the pestilential character of the atmosphere, if it be impregnated with miasmata, destroy the sources of local miasmata, and mitigate the virulence of contagion.

Of all the causes of this disease, the atmosphere, if it be most beyond our control, is, fortunately for humanity, the least to be dreaded. That it is occasionally concerned in conveying the disease from district to district, and country to country, seems probable; but that, until fanned into fatal activity by local causes, it is but slightly noxious, is certain.

Placing, therefore, out of view the atmospherical production of cholera, how should we combat the disease when it has appeared in a particular district? Our first duty will be to investigate with care whether there exists any local source of exhalation, proceeding from animal or vegetable decomposition; and, if there does, either to destroy the source, or, if that be impossible, to remove ourselves beyond its reach. Our next duty will be to encourage those habits, adopt those remedies, and enforce those precautions, best calculated, if not wholly to destroy, materially to lessen and mitigate the contagious character of the disease.

Those habits, those remedies, and those precautions, which might materially be expected to prove, and which in other places have proved, most useful, in mitigating the character and arresting the progress of cholera, have been already detailed in the preceding pages. That the suggestions contained therein may prove of some service in arresting the progress, or in mitigating the character, of the disease, is the sincere prayer of the Author.

THE END.

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